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IV.—*Some Rough Notes for the Construction of a Chapter in the History of the Earth.*—*By* R. D. Oldham, A. R. S. M., *Assistant-Superintendent, Geological Survey of India.*

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To the coal-miner, or to the mere geological surveyor, the exact correlation of the rocks in different parts of the world is of little importance. Little does the mine-owner reck of whether his coal does or does not belong to the carboniferous era so long as it is saleable at a profit, nor need the geologist, asked to survey and report on a coalfield, trouble his head about this ; but, to one who would unravel the physics or the history of the earth, the solution of this problem may well be of paramount importance, though unfortunately often impossible of attainment; generally, one might almost say always, he has to depend on fossils, but the answers these give are often contradictory or Delphic in their obscurity; at no time should they be too literally interpreted, but, like the cutcherry gong in an Indian station, must be made the most of as the only available substitute for a more accurate timepiece. But just as in this city where there are many thousand timepieces of various descriptions, of which probably no two keep identical time, every day the time-ball falls and the signal gun is fired to let all who may be concerned know that it is one o'clock ; so in the past time-signals have been given throughout the earth, by which we can determine the contemporaneity of the strata in which their records have been preserved. Of this nature would be a wide spread glacial epoch comparable to that which in the

recent past has affected both hemispheres of the globe, but, as there is reason to believe that such have occurred at various periods in the history of the earth, we are dependent on the otherwise less accurate palæontological evidence for determining whether the strata shewing signs of glacial action can have been deposited at the same period or must belong to widely separated geological epochs.

There can be no doubt that of all forms of palæontological evidence the most trustworthy is that afforded by the marine mollusca. Inhabiting as they do an element of more uniform temperature, and of which every part is in continuous if circuitous connection with the rest, it is but natural that they should be more uniform in character than the fauna of the land, while the simplicity of their structure, greater than that found among vertebrates or higher invertebrates, renders them less liable to change through alteration of the conditions under which they live. On the other hand, this very stability of organism renders them useless for the exact correlation of strata far separated from each other; for mere determination of homotaxy, even did this exist in the sense in which the term was originally intended to bear, would be but of little value to the physical geologist, to whom the terms 'Jurassic' or 'Carboniferous,' if determined merely on palæontological grounds, are as meaningless, for determination of dates in the history of the earth, as the analogous terms 'Stone Age' and 'Bronze Age' are for determining periods in the history of the human race.

But, if the evidence afforded by marine mollusca is not sufficiently accurate and trustworthy, how much more is this true of that afforded by the terrestrial fauna and flora. True, the duration of the existence of a species of cycad, conifer, and, possibly, even a fern may be shorter on the average than that of a species of mollusc, and to this extent it may be a more accurate index of contemporaneity; but it is comparatively seldom that identical species are found in far separated deposits, and palæontologists have consequently to depend mainly on what are called 'allied species.' Now the hard parts of animals, which in almost every case are all that are preserved to us, give, for the most part, a very true and real indication of the affinity of the animal to which they belonged, while, from the leaf of a tree or the frond—generally barren or with the fructification obliterated in fossilization—of a fern, little or nothing can be gleaned of the relationship of the plant to which it originally belonged; thus no one would doubt that two specimens of *Terebratula* or Ammonite, declared by a competent palæontologist to belong to the same species, would, if we could recover their soft parts, still prove to belong to the same or very closely allied species, while, on the other hand, we have lately been informed, by a palæobotanist whose competence none

can doubt, that the Indian and Australian forms of the celebrated *Glossopteris browniana*, long believed to belong to the same species, differ so widely in their fructification that it is doubtful whether they can be included in the same family, and that they must certainly belong to different genera.*

As an instance of uncertainty of palæontological evidence, I need only quote the well-known case of the Umia and Katrol beds of Kutch, where beds containing a flora with a well-marked Lower Oolite facies overlie other beds in which the fauna is equally distinctly Upper Oolite in type; another case that might be quoted is that of the Rajmahal and Damuda floras; in the Rajmahal flora, there are, out of 47 species† in all, 26 which are identical with or allied to‡ European species: of these, *fifteen* are represented in the Rhætic beds of Europe, one species being hardly distinguishable from the European form: seven are represented by Palæozoic species, two belonging to an exclusively Palæozoic genus *(Eremopteris)*, while another *(Macrotæniopteris lata)* is, on Dr. Feistmantel's own admission, so like the Permian *Tæniopteris abnormis* as to be almost undistinguishable: two species only are allied to Liassic forms, and of these one is also represented in the Rhætic: five species are represented in the Lower Oolite of Europe, two by identical forms, while, of the other three, one is also related to a Carboniferous, and the other two to Rhætic, species. From this, an impartial observer would be inclined to place the flora as certainly not later than Rhætic, but, as on this point the talented palæontologist of the Geological Survey has expressed a very positive opinion that the flora is Liassic in facies, I must perforce

* Palæontologia Indica, Fossil Flora of the Lower Gondwanas, Vol. III, p. 103 In this connection, I may quote Dr. Feistmantel as follows:—after noting the difference in the fructification of the two forms, he adds 'so that I would be quite justified in placing these in a separate genus altogether and thus disposing of the difficulty in determining the age of our Damuda series owing to the correlation of the Indian Australian species." An easy way of 'disposing of the difficulty' forsooth, but my colleague can hardly have perceived the full force of these words when he penned them, for, carried to their legitimate conclusion, they cut away the ground on which alone palæobotanists can base their claim for the acceptance of fossil plants as a means of correlating distant deposits. The lesson to be learnt is rather that the conclusions of even the ablest palæobotanists must, owing to the nature of the material they have to work with, be received with caution, and that generic and specific names of fossil plants do not necessarily represent any real affinity, and that in some cases the latter and in most cases the former are names merely and nothing more.

† Here and elsewhere, except where the reverse is distinctly stated, I owe my palæontological facts to Dr. O. Feistmantel's writings in the publications of the Geological Survey of India.

‡ I use this term in the same sense as it is used by palæobotanists; it may well be that some of these 'allied species' have no real connection with each other.

bow to his opinion, a feat I can the more easily perform that the exact determination of the age of the Rajmahal series is irrelevant to my present purpose, this being merely to point out that the flora, judged by European standards, is of an extremely heterogeneous character.

Turning now to the Damudas, we find that, out of a total 63 species, only twenty shew any affinity to European forms: of these, six are represented by Rhætic species, two of which are identical in Europe and in India: eight are represented in Jurassic beds, one being identical with a species from the Yorkshire Oolite, and two have their nearest allies among living forms: while, of those which are related to species older than the Rhætic, two are represented in the Permian, and two only are represented by allied species in the Trias. The flora of the Damudas is thus seen to be as heterogeneous in its character as that of the Rajmahals and, like that of the latter, would naturally be attributed to a Rhætic age, yet the two series are not merely separated by a break in the stratification, but the two floras are so contrasted in their characters that, whereas the Damuda flora is almost exclusively composed of ferns, that of the Rajmahals is markedly the preponderance of cycads, and, of all the Rajmahal species, three only are represented in the Damudas and those by "allied species." These beds have been classed by Dr. Feistmantel as Triassic, and the probabilities in favour of their being contemporaneous in the Trias of Europe are about the same as those in favour of a Liassic age for the Rajmahals or a Rhætic age for either of the two, but this is all that can safely be said.

Turning now to the Kach flora, which, whether we judge from the associated marine fauna or from the flora itself, is of Oolitic age, we find, out of a total of 27 (excluding Algæ) species, 18 are represented by identical or allied species in Europe, *four* are identical with European Oolitic species, of which, however, one ranges down to the Rhætic, nine more forms are related to European Oolitic species, while four only are related to species older than the Oolite and in two cases at least the relationship is not very close; we have here, then, a much closer relation with a definite European flora than is the case with the Damuda and Rajmahal beds, and this, as I shall presently shew, is of considerable importance in unravelling the history of the Gondwana age.

In Australia, there is a series of plant-bearing beds whose flora shews many affinities with that of the Indian Gondwanas, but which range over a more extensive period of time, and are marked, both at their upper and at their lower limits, by the association of the plants with marine fossils.*

* *Conf.* principally Rev. W. B. Clarke, Remarks on the Sedimentary formations of New South Wales, 4th edition, and Dr. O. Feistmantel in *Palæontographica*, 1878 (Appendix).

At the base of the series, are beds whose marine fauna indicates a Devonian age; above these, come beds which contain a flora consisting principally of such genera as *Lepidodendron*, *Rhacopteris*, and *Calamites*, among which occurs a single species of *Glossopteris*.* Above these, but still below beds in which a marine fauna of Carboniferous type is found, there is a flora which, judged by European standards, is Mezozoic in facies. At the top of the Newcastle series, to which the beds just mentioned belong, a more abundant flora is found, which presents many relationships to that of our Indian Damudas: in both, *Glossopteris* is a dominant type, both contain the *Glossopteris browniana* and two other species allied to Damuda forms: *Sphenopteris*, which in the Newcastle beds is represented by six species, is only represented in the Damudas by one (*S. polymorpha*, Fstm.), which, however, is said to be more closely allied to the Australian *S. alata* than to any European form: the only species of *Phyllotheca* is allied to the Damuda *P. indica*, and the common occurrence of *Vertebraria* in both is another link. That this relationship is not so close as was at one time believed, I readily admit, but nevertheless the relationship is real, and, though it may be presumptuous to express an opinion at variance with that of the talented palæontologist of the Geological Survey of India, I must say that to me the relationship seems far closer than that which unites the Damudas to the Trias of Europe.

Above the Newcastle beds, come the Hawskbury beds, which have yielded but two species of ferns, one of which (*Sphenopteris alata*, Bgt.), however, is allied to a Damuda species. Above the Hawksbury, come the Wianamatta beds, which have yielded six species of plants, no less than three of which are allied to Damuda forms.

It is thus evidently impossible to correlate, on palæontological grounds alone, these beds directly with any of our Indian horizons, but, like the Indian Talchirs, the Hawksbury beds contain certain beds of fine clay through which boulders of all sizes are scattered promiscuously in a manner that can only be attributed to the agency of floating ice. In Victoria, there are beds which similarly indicate the existence of a severe climate at the time of their deposition, and these—the Bacchus Marsh beds—have yielded three species of *Gangamopteris*, of which one is identical with, and the other two are closely allied to, Talchir species. The Bacchus Marsh beds have not yielded a single species common to themselves and to the Hawksbury beds, but this is of little importance, as it is impossible to suppose that the entire flora of the Bacchus Marsh period consisted

* There is some doubt attaching to the correctness of this statement. The *Glossopteris* was obtained from a different locality and possibly from a newer series of beds than the others.

of three species of *Gangamopteris*, or that of the Hawksbury period to have been limited to two species of ferns. But, if not directly referable to the same epoch by their contained fossils, there can be no doubt that they are on the same horizon, for, in the uppermost beds of the Newcastle series, two species of *Gangamopteris* are found, one identical with, and the other allied to, species from the Bacchus Marsh sandstones of Victoria, while the beds above the Hawksbury series in New South Wales can be correlated with those which overlie the Bacchus Marsh beds in Victoria by the occurrence of *Pecopteris australis*, Morr. and *Tœnopteris daintreei*, McCoy in both. The presence of beds indicating glacial action in both and the absence of similar beds in the associated strata further prove their absolute contemporaneity; and by an extension of the same reasoning we may assign the Talchirs of India to the same glacial epoch.

The palæontological relations of the Gondwanas with the Karoo and Uitenhage series of South Africa are much simpler than with the Australian formations. From the upper part of the Karoo beds, which unconformably overlie strata containing an Upper Palæozoic fauna, a limited flora of but five species has been obtained. Of these five, one is *Glossopteris browniana*, another, *Dictyopteris ? simplex*, Tate, is, according to Dr. Feistmantel, allied to *Glossopteris damudica*, Fstm., and *Rubidgea mackayi* is, on the same authority, probably a *Gangamopteris;* in addition to these, Tate gives a species of *Phyllotheca*, but the identification is doubted by Dr. Feistmantel.* Associated with these, there is an abundant and peculiar Reptilian fauna with *Dicynodon* as a dominant type, a genus not known elsewhere, except from the Panchet subdivision of the Damuda in India. In the overlying Uitenhage series, there is a flora consisting of eleven determinable species; of these one species of ferns is also found in the Rajmahals, while two, and possibly three, species of ferns and one conifer are closely allied to Rajmahal forms.† These Uitenhage plants are associated with beds containing an Oolitic marine fauna. The palæontology of these beds sufficiently indicates a parallelism with the Gondwanas, and, in confirmation of this, we find, at the base of the Karoo series, an undisputably glacial boulder bed,‡ which we shall be justified in assigning to the same epoch as those of the Talchirs in India and of the Hawksbury and Bacchus Marsh beds in Australia.

Viewing these circumstances, there can, I think, be no doubt that these glacial boulder clays of Africa, India, and Australia represent one and the same epoch in the history of the earth and are, as strictly as the word can be applied, of contemporaneous, if not absolutely coeval, origin.

* Q. J. G. S., XXIII, 140, *Palæontographica*, 1878, p. 114.

† Q. J. G. S., XXIII, p. 140.

‡ Q. J. G. S., XXVII, 58 and 535.

And further, as in every case the palæontological evidence indicates that these glacial beds are of late Palæozoic or early Secondary age, I think it is probable that, as has been suggested by Mr. H. F. Blanford, they are of the same age as the Permian boulder clays of Europe.*

Having thus obtained a common era in the geological history of these three countries (India, Africa, and Australia), we are able to examine their history in an intelligent manner. The first thing noticeable is that, in Australia, at a period corresponding fairly to the Devonian, both the fauna and the flora were, judged by European standards, of a Palæozoic type. Later on, probably in Lower Carboniferous times, there appears, among species of *Lepidodendron*, *Rhacopteris*, and *Calamites*, which, in Europe, are found in rocks of Carboniferous age, a single species of *Glossopteris*, the forerunner of a newer flora destined to supplant the older forms. In the Newcastle (Upper Carboniferous) beds, this flora has completely ousted the older forms, and, as I have already noticed, shews considerable relationship to that of the Damudas in India. Yet, if the Talchirs and the Bacchus Marsh beds are really of contemporaneous origin as was first suggested by Dr. Feistmantel, and if the Bacchus Marsh and Hawskbury beds are also contemporaneous (and the presence of traces of glacial action in all three is at least presumptive evidence in favour of this conclusion), the Damudas must be of very much later date than the Newcastle beds, and we have to explain why it is that the Newcastle flora left Australia when it did, and why it or its descendants lingered on in India, and, as I propose to shew, spread over what is now the Old World producing important modifications in its flora.

It is possible to suppose that the Newcastle flora required a warm —though from internal evidence one would rather look upon it as indicating a cool temperate—climate; that, on the advent of more severe conditions, it migrated towards the Equator and remained there, not merely through a period of extreme severity, but through a further period, when the climate was cooler than it had been during the deposition of the Newcastle beds, and during which a flora more suited to the latitude flourished in Australia. But there are so many objections to this hypothesis that it can hardly be tenable, and, however wild my alternative hypothesis may be thought, I hope to prove that it is really the more probable of the two.

In the first place, we have to account for the prevalence of glacial conditions at a low level in India even within the tropics. This was not paralleled during the last glacial period, for even the erratics of the Petwar are 10 degrees beyond the tropics and 2,000 feet above the level

* This correlation of the Indian, African, and European boulder beds has been suggested by Mr. H. F. Blanford, Q. J. G. S., XXI, p. 519.

of the sea, while the Petwar was certainly not less elevated during the glacial period than it is now. Further, the glacial deposits in India are far better developed, and, to judge from the descriptions, must be far thicker and represent a much longer period of time during which the climate was severe than those in Australia. Yet the glacial deposits of New South Wales are 10° further from the Equator than the Indian, so that, if we might shift the Equator some 10° further south between India and Australia, observed facts would be more in accordance with what one would expect than can be the case if we are compelled to assume the Equator fixed throughout all time.

But, if we try to compare the facts observed in Australia and Africa, we are landed in a still greater difficulty, for, lying as they do on about the same parallel of South Latitude, the glacial beds are more strongly developed in Africa even than in India; and, as we can hardly suppose the greater severity of climate to be due to altitude, it must have been due to latitude, to obtain which we must suppose that that portion of the Earth's crust which now forms South Africa then lay in a higher latitude than that which is now Australia; in other words, the comparison of the Permian (?) glacial beds of Africa and Australia, as in the case of Australia and India, points to the conclusion, either that there has been a change in the position of the axis of revolution of the earth, or, what is more probable, that the crust of the earth then occupied a position relative to the central nucleus different from that which it now does. An experiment with a globe will shew that the relations of India, Australia, and Africa indicated above, *viz.*, that Central India was in a higher latitude than New South Wales and South Africa in a higher latitude than either, are best satisfied by taking the Equator between India and Australia, but nearer the latter than is now the case, and thence through a point lying between the Cape of Good Hope and the South Pole in not less than 70° of South Latitude; a disposition which would bring some point in Central Africa over one of the poles.

Turning now from these physical and climatic arguments to those derivable from palæontology, I hope to shew that they lead to the same conclusions.

I have already referred to the fact that the Damuda and Rajmahal floras of India shew affinities with those of almost every division of the Mezozoic era in Europe, and I would now draw attention to the fact that those species which are related to upper Secondary forms in Europe belong very largely to types which first appear in the Palæozoic beds of Australia. Foremost among these, of course, are *Glossopteris*, *Phyllotheca*, and *Vertebraria;* not known in Europe before Jurassic times, these were certainly living in Australia at the commencement of the Carboniferous epoch. *Pecopteris*, *Thinnfeldia*, *Gangamopteris*, *Nœggerathiopsis*

likewise are found in the Newcastle series of New South Wales, but in Europe only in Secondary beds. Allowing that some of these genera are purely artificial, and that the species grouped under them may not really be allied in every case, it is on the other hand probable that some forms placed under distinct genera should properly be united with some of those grouped under the genera above mentioned, and, making the most liberal deduction for the value or want of value of negative evidence, I think that there is still a very considerable weight of probability, on this count alone, in favour of a newer type of vegetation having originated in Australia in Palæozoic times and in the Permian period commenced to spread over the rest of the world.

The explanation seems to be that, on the advent of the Glacial period, the flora, which had supplanted the older types in Australia, was driven towards the Equator. As the climate ameliorated, it did not again retreat towards Australia, either because its place was taken by newer species, or, more probably, because, owing to changes in the distribution of land and water, it could no longer do so, but to the north—or what for convenience we may provisionally call the north,—of the Equator it lived on in what is now India and, gradually spreading over the hemisphere, produced a profound modification in the pre-existing floras of what we now know as the Old World.

The flora of the Wianamatta beds, as I have explained, shews a certain relationship with that of the Damudas, but none with that of the Newcastle beds as far as species go; of the genera, however, three out of the six, or, if we include the Hawksbury beds, four out of seven are also found in the Newcastle beds. The beds newer than the Wianamattas have yielded a flora consisting of nine species belonging to seven genera, of which, if we except the *Phyllotheca australis*, only one species is allied to an Indian form, *viz.*, *Pecopteris australis*, Morr. allied to *P. indica*, Oldh. and Morr. from the Rajmahals. We have here a distinct decline in the closeness of relationship between the Indian and Australian floras, and, though, of course, this might be due to the imperfection of the record, the probabilities are against its being entirely due to that cause, and we may safely conclude that some barrier separated the two areas, by which the floras of India and Australia were kept apart and followed separate and consequently diverging lines of descent.

Turning now for a while to South Africa, I must commence by declaring it as my opinion that the relationships between the Indian and African floras of the periods I am discussing are with difficulty explicable, unless it is granted that there was in those days a continent, or at any rate a continuous chain of islands, stretching from South Africa towards India. I am aware that Mr. A. R. Wallace has declared such to be uncalled for and

impossible to grant,* and I am ready to admit that the facts of distribution of animals as detailed by him are conclusive against the possibility of such a distribution of land and water, at any rate since the Miocene period. But there is no reason to suppose that the present distribution of plants or animals can throw any light on the distribution of land and water in late Palæozoic and early Secondary times. On the other hand, in favour of the land connection, I claim, *firstly*, that the relationship between the fauna and flora of the Damudas on the one hand and the Karoo beds on the other is far more real and close than the mere 'similarity of animal and vegetable productions' to which Mr. Wallace seems to have considered it to be confined; *secondly*, that this relationship of the two floras continued into the Uitenhage and Rajmahal series, which could hardly have been the case had the two areas been as separated then as now; and, *thirdly*, that the very peculiar relationships and differences between the cretaceous faunas of Central and Southern India on the one hand and Arabia and South Africa on the other are such as imperatively to demand the existence of a continuous barrier of dry land stretching between India and Africa. It is needless to expatiate further on this point, for, if such a barrier existed during the Cretaceous period, any argument against its possibility derived from the doctrine of the permanence of continents must fall to the ground, and there remains no reason why, if on independant grounds its existence is shewn to be probable, such a modification as I require may not have existed at the commencement of the Secondary period. That, during the deposition of the Damudas, there was continuous land communication with South Africa I do not suppose, for the very remarkable reptilian fauna, which, like the recent marsupial fauna of Australia, mimicked many of the higher mammalia, points rather to some isolated continental island which was connected with India, as Australia now is with Asia, by a chain of large islands separated by narrow straits, across which the spores of ferns and the seeds of plants could be wafted, but which were impassable to terrestrial reptiles.

But even a land connection of this sort would probably be inadequate by itself to account for the close relationship which the small fragment preserved to us of the flora of the Uitenhage period shews to that of the Rajmahals. For it is at least highly probable that the heat of the Equator would be as effectual a barrier as a broad sea, and, if the floras of India and Africa had pursued independent courses of development for a period sufficient for the dying out of every species and almost every genus, and for a change in the facies of the flora from one composed mainly of ferns to one composed mainly of cycads, it is inconceivable that the floras of the Uitenhage and Rajmahal series should exhibit the close

* Island Life, p. 398.

relationships they do. But this difficulty would not exist could we suppose that what are now South Africa and India then lay on the same side of the Equator; and thus the palæontology of these beds, as well as their petrology, points towards the conclusion that in early Secondary times the crust of the earth did not occupy the same position with respect to the axis of rotation as it now does.

That none of these arguments are conclusive by themselves I admit; I willingly admit that the floras preserved to us represent but a fraction of the species that lived when the beds that have yielded our fossils were being deposited, but the probability is vastly against only those species which were related to each other in the two countries being preserved, and we may, I think, safely argue from the small sample preserved to the larger bulk which is lost. In the same manner, I freely admit that the differences in the severity of climate may have been due to other causes besides difference of latitude, but on the average a colder climate indicates a higher latitude, and, when we find that, from whatever point we approach this matter, we are led towards the same conclusion, it seems to me that there is a very strong presumption in favour of its truth.

I fear this paper has already extended to too great a length for me to examine the arguments that have been put forward to prove that any change of latitude is physically impossible, but I cannot conclude without pointing out that what has been proved is that no conceivable elevation or depression of the earth's surface could produce an appreciable alteration in the axis of rotation of the earth as a whole. But, though the mathematical reasoning on which this conclusion is based may be unassailable, it has no bearing on the question of whether changes of latitude may not have taken place in the past, except on the assumption that the earth is rigid throughout, and that the crust has no power of sliding over the heated if solid core, an hypothesis which has been ably combated by the Rev. O. Fisher,* and which I hold to be inconsistent with the known facts of stratigraphical geology. While, if the views put forward in this paper are true,—and there seems to me a very strong presumption in their favour,—the crust of the earth must in Mezozoic times have occupied a very different position with reference to the axis of rotation from that which it does at the present day.

As yet the only fact which has in any material degree attracted the attention of English geologists is the prevalence during the past of mild climates within what are now the Arctic regions; and hypotheses have been broached to account for this independent of an alteration of the position

* Physics of the Earth's Crust *passim*; see particularly p. 184.

of the crust relative to the central core of the earth; but the more completely such an hypothesis may explain the absence of any trace of glaciation in the Palæozoic, Secondary, or Tertiray rocks of the Arctic regions, to which Baron Nordenskjöld has drawn our attention, the more irreconcileable is it with the repeated traces of glacial action that are met with almost within the tropics. Yet the latter as urgently requires explanation as the former, and I have put these suggestions forward not from any conviction of their intrinsic truth, but because I feel that the rigid bonds within which mathematicians have sought to confine geologists must be largely and materially relaxed, because I feel that every addition to the growing pressure against these bonds is of some—even if but small—importance, but chiefly because I trust that I may be instrumental in drawing the attention of others with greater opportunities and greater abilities to the solution of this problem.

P. S.—Just a week before this paper was read Mr. W. T. Blanford, addressing the geological section of the British Association at Montreal, devoted the greater part of his address to the consideration of a subject to which he has before now referred, more particularly in the Records of the Geological Survey of India, and on which I have cursorily touched in the introductory part of this paper; I mean the uncertainty of palæontological evidence in determining the exact correlation of widely separated beds. He also refers to a report on the Stormberg coal-fields by Mr. E. J. Dunne, which I have strangely overlooked: Mr. Dunne mentions the existence of three species of plants in the Stormberg beds identical with Australian species, an identification which, if correct, greatly diminishes, if it does not altogether annihilate, the value of my argument from the relationships between the African and Indian early Secondary floras, but this is of the less importance, as, owing to the known value or want of value of negative evidence in palæontology, little value would in any case attach to an argument of this kind.